

McNAHARA

~~CR/CS~~ ~~DES~~

No McNamara

Testing

Jan 21 63

NUOS EWH

(MAO coming)

Cost projections: "low wave" effect — peaking of costs in years following the budget year and a sharp tapering off in later years.

Peaking because: 1) Postponement to ^{the} next year of marginal + less urgent projects; 2) fact that programs costs beyond FY 64 have not been subjected to the detailed + rigorous budget reviews accorded the 64 estimates. Thus, we are continually pushing the peak of the program before us from year to year; "low wave" effect.

Sharp downward slope after FY 66 simply reflects our inability to see very clearly the course of future events. Typical downward bias inherent in all longer-range projections, gov or industry. (future decisions to produce + deploy for some projects not reflected)

C&C

Gen. Terkune : ESO

Col. Townsend, head of Photo Programs at ESO

Charlie Brackett, planning at MITRE.

See Clark, Stewart

Prim

See Fletcher Report (see Col. Frank Yeager

AFOAC; on committee following up Report).

It will become increasingly difficult, regardless of the form of the attack, to destroy a sufficiently large proportion of the SU's strategic nuclear force to preclude major damage to the US, regardless of how large or what kind of strategic forces we build. Even if we were to double ^{and} ~~or~~ triple our forces we could never hope to destroy all or almost all of the land-based ICBM sites in the very first blow. And even if we could do that, we know no way to destroy the enemy's missile launching subs at the same time.

~ Moreover, to minimize damage to the US, such a force would also have to be accompanied by an extensive missile defense system and a much more elaborate civil defense program than has thus far been contemplated. Even then we could not preclude casualties counted in the tens of millions.

The most likely possibility is that we would have to strike back after absorbing the first blow. ...

This means we have to build & maintain a second strike force.

In talking ^{to} ... 1) strike back decisively at entire Sov target system simul., or 2) strike back first at ... to reduce power of any follow-on attack — and then, if necessary, strike back at Sov urban & industrial complex in a controlled & deliberate way.

In talking about global nuclear war, the Sov leaders always say that they would strike at the entire complex of our mil. power, inc. gov & prod centers, moving our cities. If they even to do so, we would, of course, have no alternative but to reply in kind. But we have no way of knowing whether they would actually do so. It would certainly be in their interest as well as ours to try to limit the terrible consequences of a nuclear exchange. By building into our forces a flexible capability, we at least eliminate the prospect that we could strike back in only one way, namely, against the entire Sov target system, inc. their cities.

Such a prospect would give the SU no incentive to withhold attack against our cities in a first strike. We want to give them a better alternative. Whether they would accept it in the crisis of a global nuclear war, no one can say. Considering what is at stake, we believe it is worth the additional effort on our part to have the option.

In planning our second-strike force, (destroy all soft and semi-hard military targets and a large number of their fully hard, + protected reserve force.

We have not found it feasible, at this time, to provide capability for ensuring the destruction of any very large portion of the fully hard ICBM sites or missile subs.

Fully hard ICBM sites can be destroyed but only at great cost in terms of the numbers of offensive weapons required to dig them out. Furthermore, in a second strike situation, we would be attacking, for the most part, empty sites from which the missiles had already been fired.

[How many advanced MM needed to get

50-100-300 psi SU land missile? 2.5 MT, 3000 ft CEP

.8 SSKP for 100 psi .5 for 300 psi. ~~94B~~

Why would land missiles already have been fired?

Don't SU have need for reserve?

Are they firing against US MM?

~~How~~ How about first strike?]

9 MT, 1 mi CEP: 35% SSKP vs 300 psi.

$\frac{1}{2}$ mi CEP: 78% vs 300 psi

with .8 mt., takes 5 weapons for .9 kill.

The value of trying to provide a capability to destroy
a very high proportion of low land ICBM sites becomes
even more questionable in view of the expected increase
in the low missile launching sub force. . . . once the
SU places any large number of them on station.

Although SU may now have, or soon achieve, cap. to place ~~these~~ ^{these} homed. sats in orbit...

there does not appear to be any logical reason for them to do so, since there are much more efficient ways of delivering nuclear warheads. [!]

But we cannot now ignore the possibility of that kind of threat arising in the future...

Modifies Nike Zeus on Kavarado to give it anti-sat cap. by May, 1963 (up to 200 mi).

Modifies Thor on Johnson Island to kill up to 700 mi.

by end FY 64

funds in AF budget for non-muc system.

Interim AICBM program: might reduce casualties from a "small" or "medium" attack on urban areas [e.g. SU retal]

also, complicate design + tactics of attack. [15th countries?]

also, it is possible, although not probable, that the Soviets may not pursue an extensive program of penetration aids, even though it would be very much in their interest to do so.

[No mention of prestige, prop. or counter to SU sub missiles]

562 m for research (327 in 63, 287 in 62)

1,172 for exploratory devel.

range of those not wildly incompatible with evidence as to what they were building. Even lowest estimates of what they would build were ~~too~~ very high, in retrospect; nor were they the lowest of the range of estimates compatible with the evidence, which obviously was "compatible" with much lower estimates.

[Was the evidence such as to cast strong doubt on the correct hypothesis: that they would not build G's?]

[What sorts of evidence were relevant? 2) tests, R+D, construction, production?

- b) Space program
- c) Past patterns of test, production, deployment
- d) Statements.
- e) Evidence bearing on SU past military/political objectives, doctrine
- f) Calculations of "objective" payoffs to various sorts of forces.

(Distinguish between "negative evidence" in sense of: a) evidence tending to discredit a hypothesis; b) evidence supporting a

NUC NOTES

On the missile gap — JAN 1963

3) Most startling feature, is not the general emphasis on the possibility that the SU would build lots of ICBMs fast, but the total absence of opinion, conjecture, even mention of the possibility that they would ~~not~~; ~~at~~ ~~least~~ build few, late (in a period when their value was much less than earlier).

5) The estimates were always based on "intention", not "capability." In early period, "capability" provided a limit on what they "would" build; it was believed that they would build all they "could" (a question whether "capability" should be defined by a program accepting the costs of a crash effort or not). Later, ^{when it became available,} evidence on what SU could build was evidence on what they were building and testing — not evidence on what they could build, in terms of R&D, GNP, etc.

Even highest estimates were less than estimates of what they could build. DSAF estimates were at extreme of the

Almost all intelligence data relates to current reality:
predictions what go beyond evidence, based on theory.

~~INSP VCS counts 7 Dec~~

o) Hard to communicate basis for beliefs in low programs
to public because they weren't closely related to confidence
in the adequacy of the overall collection program and
hence the significance (weight of evidence) of the
lack of positive indications — "our sense of what we
might have missed" — which became predominant evidence
in 1960-61: evidence strong enough to have dominated any
prior opinions except extremely strong ~~sp~~ belief in big SU
program (i.e. USAF; Army/Navy started with some general
beliefs). Basis for this confidence could only be exposed
by revealing the scope of the entire program. No one
element would be enough: except the U-2, which, when
revealed, did validate Administration statements about
1959-May 60 (Would also have suggested 1:6 odds on
150 SU missiles if he had known of flights of U-2 in Nov 59?
as VCS did?) [Who at RAND knew?] [Even then, you would have
to know why U-2 coverage was "incomplete," and what the quality
of the photography was]. Not available, to persuade public (of Cuba)
after May, 1960. [In Cuba, easier to see photos to "prove" presence
than to prove "absence."]

Thus, hard to "use" some intelligence because appropriate action would seem foolish or reckless to public or Allies or even, to enemies (of they didn't know info that action was based on).

"Negative" opinions particularly hard to communicate, without exposing the general goodness, precision, of the intell. program (a single "positive" indication can be attributed to "luck" with an obviously ~~unreliable~~ ^{spotty} source: e.g. a tourist photo).

Thus, to make opinion plausible at all, proponent must try to "infer" it from the evidence available to the public: a process that may suggest wildly foolish or over-confident prior probs, or debatable notions of "evidence." (E.g., arguing as to current posture purely from assumptions on "intentions.")

Moreover, this evidence cannot be used to discredit the claimed, perhaps honestly believed, opinions of, e.g. USAF, as to future posture (AFCM could be stopped from distorted statements about present, but not about future)

"negative hypothesis": that certain objects do not exist within some region, that certain events did not occur, that a relation does not hold, etc.; ϕ absence of evidence strongly confirming a "positive hypothesis," ^{though} ~~but~~ also lack of evidence

c) data that is "unlikely" both by a given hypothesis and by its complement.

Note Army/USAF dispute.

c) Was not merely a preoccupation with "worst" case: e.g.

1) technical characteristics could have been worse

2) IRBM/MRBM could have been bigger program (even with big ICBM program? With CIA, or Army program?)

d) Opinions as indicated by small side bets would in some cases have corresponded to official estimates, in other cases not.

e) Costs of wrong estimate — not just of SO ICBM inventory, but of SO intentions, values, preoccupations, doctrine — are not to be measured solely in ^{US} ~~miss~~ overemphasis on missiles and strategic forces but mainly on underemphasis on other matters, in terms both of resources and of intellectual attention. It is not the money that was spent on missiles — that may not have been wasted at all, in the sense that even the last dollar spent may have had utility > 0 ; cost is ~~is~~ measured in terms of ~~the~~ alternative ways that could have been made of the money, + time, effort + intellectual attention. [Of course, we can't assume that money would have been spent in a particular way, or a better way]. To assume that cost was not serious is to assume that, although we were adapting to a very false image of the world + our future, we "luckily" used our resources very wisely in terms of the world that ^{or, as wisely as if we had had a correct image of} did exist. This is less plausible w.r.t. intellectual effort than with money (given reluctance of like to spend money on anything but the missile gap). E.g. alliance policy...

f) Question that Rusk & Hornick I reply a very misleading answer to (partly, by appearing to raise it and then not saying much to contradict reader's probable prior probs) is: Why were we so wrong in our estimates? (R+H: because

we were uncertain, lacked evidence other than statements, and were "fooled" by K statements).

g) Relative "strength" and pattern of prior probs, in interpreting evidence.

h) Why were we so wrong in predicting future SV deployment when ~~we~~ with respect to current deployment we a) had lots of evidence, b) were not, until mid-60, divided or uncertain, c) till mid-60 were basically right? (Here, decision-makers differed drastically from public, Allies).

i) Note that it was not inevitable, in June 61, that CIA would stick to their estimate of 100, or that Army/Navy would change theirs; and note, even USAF estimate (and any estimate) is less than would be predicted by normal growth from 100.

j) Note now, relative lowness even of USAF long-range projections.

k) What was position of SAC Int during period? Influence?

l) Can't explain prediction easily in terms of ~~some~~ "national"

Service interest : USAF's was too high to fit its doctrine, Army/Mary was too low (Note divergence from Taylor's book, issued in Feb 60). ^{some} Service bias : perceived interest, preoccupation, mirror image (of Service, not "US").

m) "Conventional gap" may have been overestimated almost as much. But ~~was~~ here, not a failure of prediction but of current estimate (though, less ^{intell.} resources here?).

n) Cost of overestimating Hitler's AF was Munich. Imagine a Cuban crisis — SU missiles in Cuba — in April 1961, before the new intelligence.

Confidence

c) Imagine an "experiment" — a "data generating process" — relevant to current opinion. What prob do you assign to data that would drastically change your current opinion? Answer may be 0: high "confidence" in current opinion. Or, it may be high, .8, or even 1. That is low confidence in any particular set of probs assigning high prob to one hypothesis. Both states of mind might be represented by a uniform dist — will action be the same? (in terminal decision?).

Often we express an "opinion", and even act on it in less important matters, "assuming" that certain conditions ^{calculate,} hold, or hold approximately. I.e. we express & act upon opinions ~~that~~ as if we thought the premises of a certain model were certainly true or approx. true — even though we may not be certain of this. We can be treated for the prob we assign to our model, the premises

that underly our stated "opinions": this may be high or low. If a certain model is usually assumed in common between speaker & listener (i.e. usually taken ^{that they share certain premises} for granted) it can be very misleading to express a prob dist over "events" without revealing that much of the uncertainty ~~is~~ concerns the model, or the relative certainty relative to a different model. (different hypotheses considered, different "likelihoods"). See Box & Tiao.

Our beliefs as to the models that would apply if our "usual" one does not may be clear-cut, and likewise our opinions as to their probs; or both may be very vague.

In the first case, we may tend to act in important matters as if we had a definite ~~uniform~~ prob dist over events; in the latter, we may act as if our opinions were "vague"; although in both cases, if calculation is much easier with "standard" model (or, if we have some reason — but not over-riding — to reinforce belief in standard model — to demonstrate loyalty, deceive opponent, express identification, further coordinational action, influence others' beliefs or actions or expectations of us) we may "reflect" ~~reflect~~ definite, sharp opinions in less-important actions. (I also, utility effects).

In most of our reasoning, we have to adopt a simplified model: i.e. reason ~~(act) as~~ as if ("assuming") certain things were certain, or impossible. We may be almost certain that at least one of these premises is significantly wrong, though we attach fairly high prob to each one.

(The chance that 10 premises, each .9, all hold is . 3^4)

AW: start from premises, model, in which you have high confidence: e.g. weak premises about range of possibilities.

(me: & don't be very confident even of those, if they concern R+D, energy actions, energy payoffs & expectations, etc.)

[Note possibility, when it is hard to predict effects, ~~even~~ ^(particular) points of ~~given~~ energy or friendly actions, or the set of actions available, to define a set of results, outcomes, situations ~~some~~ of which, you are confident, will emerge, and go on from there.

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Thus, we often express what are really, even consciously, conditional probes over events, "as if" they were "unconditional." There may be opinions we "refuse to back," opinions on which we fail to act when important consequences are concerned. We show a preference for "safe" actions, or "status quo" actions, or actions consonant with "conventional," "scripted wisdom" (which, in case of failure, will not expose us to criticism of having acted foolishly, as opposed to ~~acting~~ acting on our expressed, "foolish" opinions. If we did the latter, and won, we still might not get credit; "just back, at terrible risk" — like in missile gap.

"I think that A is ^{much} more probable than B" ~~often~~ may mean that "I think it more likely than not that a set of conditions obtain approximately such that, 'given' those, I think that $A > B$." Oh! I may not be willing to back this.

Strength of opinion.

- 2) What data would change my opinion? ^{b)} What is the ^{my} prob of such data emerging from a given process?
- 3) What is my prob that process will occur, and I will learn result?

Some beliefs may be inferior to "evidence" from a given process because: 1) It they assign equal "likelihood" (conditional probs) to results of process; or 2) they assign high likelihood to results that are likely (on other grounds) to be observed in processes likely to occur and be reported; low likelihood to results less likely to be observed, or reported, or processes unlikely to occur. This is the way to protect an opinion from facts. (Or: claim definite opinion, but extreme vagueness as to likelihood).

Note: Birnbaum would consider an experiment "uninformative" if the result observed were $P(D|H) = .000...0, \frac{1}{2} = P(D|\bar{H})$. But note the effect of this data on the "plausibility" of the model. (Box & Jenkins).

Moral: rarely attach to ^{or "calculation"} predictions of future events and relationships the same degree of high confidence that we are often justified in attaching to estimates of the present: particularly in area where

1) our models, theories are so crude, bad, so they are in political science, etc.: processes are not understood.

2) situation is so dynamic as in R & D, int. relations; important premises true today become untrue so rapidly;

3) process is so complex; ^{or inaccurate} changes in one of many pieces of data can invalidate calculations or predictions (i.e. make calculations based on former model inappropriate for or irrelevant to future situation).

i.e. avoid unnecessary commitments; retain ^{"decision"} exploratory attitude, search for informative experiments (wouldn't need these if already "knew" the answer: how "informative" is the experiment of holding a lighted match to one's finger?

Thus, Bimbaum measure based only on likelihoods is misleading) entertain hypotheses contradicting our "best guesses."

"Sometimes it is necessary to leap in the dark — to achieve a goal." But, this is not a preferred way to get where we want to go, and it is a good way to "lose" decisively.

Often we leap in the dark when we could have had light, or when we need not have leaped at all. (Leaping is always more exhilarating than groping; and it offers a higher chance of "ending uncertainty" soon, though with a higher chance of an unhappy resolution.